

Chapter 18

An Unknown Level in Carbon-12

THE PREDICTION in early 1953 of the existence of a hitherto unknown level at an excitation of about 7.65 million electron volts (7.65 MeV) in the nucleus of the common isotope of carbon, carbon-12 (commonly written ^{12}C), has achieved some notice in recent years on account of its being an early application of what is known nowadays as the anthropic principle. Indeed, it is sometimes referred to as the only, predictive application yet made of the anthropic principle. In this chapter, I will explain how, as an outcome of another lucky chain of accidents, the prediction came about.

The anthropic principle is a curious notion that appears to be clear to some people but not to others, as often is the case with concepts to which the word principle is applied. No one can quarrel with the statement that the physical properties of the universe must be consistent with our own existence, a statement so obvious as hardly to justify the term principle, one might think. Nevertheless, this trite observation can be made to do some work, as the ^{12}C story showed. Application of it to cosmology has some predictive possibilities. Thus, in evolving cosmologies, human life could not have existed in earlier times, when the temperature everywhere was too high, nor will it exist in later times, when all dwarf stars, such as the Sun, will have burned out. These two requirements set the epoch of our existence as between about 10 million years and 30 billion years after the Universe started in a big bang. If one believes in the big bang, this is not a negligible deduction. In other forms of cosmology not of the big-bang type, galaxies can be of variable ages—some old, some young. The anthropic principle requires us to live in a galaxy that is old enough for the evolution of life to have led to our existence. Should this require us to live in an exceptionally old galaxy, then that is where we must be, regardless of the comparative rarity of exceptionally old galaxies. Probabilities do not arise in such a case, as they would for a randomly chosen observer.

More subtle issues appear when the existence of life is seen to depend crucially on a fine tuning of the laws of physics themselves, in the sense that it is not hard to imagine minor changes in the laws that would have made life impossible. Thus, we are faced with several alternatives. Either our existence is a freakish accident, or the laws of physics are not invariant. Either the Universe is more complex than we think, in that variations of the laws are realized (in which case we happen to pick out the particular choices that are suited to our existence), or the Universe is teleological, with the laws deliberately arranged by some agent to permit our existence. The latter view is, of course, common to most religions, but it were better for a scientist to have a millstone hung around his neck than that he should admit to such a belief—yea, verily. If he does so, his papers will be rejected, he will receive no financial assistance in his work, the publishers of his books will receive threatening letters, and his children will be waylaid on their way home from school. As well might he seek to pass through the eye of a needle, for to hold such a view is the greatest possible scientific heresy. On the other hand, it is possible to hold the inverse view (and even to win plaudits by doing so)—namely, that it is our existence that requires the laws to be the way they are. Stated this way round, you have what is called the strong anthropic principle. Our existence dictates how the Universe shall be, a fine ego-boosting point of view on which you may travel, fare paid, to conferences all over the world.

A balanced person will be surprised to hear that a respectable argument can be given in support of this seemingly outrageous point of view. It concerns the phenomenon of the condensation of the wave function in quantum mechanics. It is possible to set up a physical either/or kind of experiment without difficulty. A radioactive atom either decays in a specified time or it does not. If it does, the decay products are used to trigger a camera, and a picture is taken of the House of Commons in session. If it does not, the absence of decay products triggers a different camera, and a picture is taken of St. Paul's Cathedral on Easter Sunday. So, inevitably, some picture is taken, and the question is, How do we discover if it is the House of Commons or St. Paul's? Not by calculation—absolutely not. Any attempt to fiddle the calculations to yield a definite prediction results in abysmal contradictions. Calculation can only assign relative probabilities to the two possible results of the experiment. The widely accepted answer to the problem, given in my youth by what was called the Copenhagen school of quantum mechanics, associated particularly with Niels Bohr and Werner Heisenberg, was that decision is made in the matter by the experimental equipment, the circumstances of the triggers, the electronic storage devices, and the cameras. Schrödinger differed, however, arguing (in what came to be called the “Schrödinger cat experiment”)

that the ultimate decision rests with the human who eventually takes a look at what the cameras have done. This was very much a minority view, as I discovered in 1938, when, in ignorance of the existence of Schrödinger's now famous cat, I arrived at a similar conclusion. The thing hit me as I sat near the village of Grantchester on the banks of the river Cam, after swimming in its polluted water, which gave me a sore throat for life. The respectable position in physics today is different. The Copenhagen school is in eclipse, with most of the younger generation tending to agree with Schrödinger. So it is human consciousness that makes an otherwise indeterminate world determinate, the process known as the condensation of the wave function. After human consciousness resolves a quantum uncertainty, the future behavior of the world is calculated for new, more specific conditions.

All this rather trumps the trick of the religious person who sees the Universe as a sort of factory set up by God. It makes more sense to suppose that a bit of God is operating in all of us, and not mainly over high-flown moral issues but even over such issues as which pictures are produced by our two cameras, when it is God who decides whether the picture is to be the House of Commons or St. Paul's. It is almost as if, without such interventions, God doesn't know what is going on in the Universe, as if this is the way that a record of happenings is kept. I pointed out earlier that one has no affection for a pulled tooth, despite the tooth having served one well by chewing up many thousands of meals. It is as if the bit of God that is in us doesn't give a hoot about the hardware by which relevant observations are made. The hardware matters only so long as it continues to work. When it doesn't, it becomes no more important than any other collection of atoms of hydrogen, carbon, oxygen, nitrogen, and so on. This is the kind of speculation, however, that is only too likely to get one's papers put into the shredder and to cause one's publishers to be hounded to extinction, as well as to ensure that one's grant applications to government agencies are thrown instantly into the deep freeze. But, in a common-sense view, it is the way things seem to be.

Yet again, I owed the prediction of the 7.65 MeV level in ^{12}C to the strange structure and history of the University of Cambridge. To understand nineteenth-century Cambridge, one should compare the research student of today with the undergraduate of 1850. The equivalent then of the modern undergraduate course was the excellent instruction given in upper forms of independent schools and grammar schools, and the equivalent of the modern B.A. degree was university matriculation, which was dependent, in Cambridge, on an examination referred to generally as the Little Go. Just as, in my day, some research students engaged in curious pursuits, like the one who spent three hours a day brushing his hair, or

the one who enjoyed consuming immense bundles of bananas, so, in the nineteenth century, there were many wealthy undergraduates who did not take university life any too seriously. We did not think the chap who spent three hours a day brushing his hair to be narcissistic, by the way. It was just that his mental processes were very slow, like those of the elephant.

And, just as modern research students have a supervisor, so undergraduates then had a coach, who was paid by the student according to his skill and reputation, with the best students wanting the best coaches and the best coaches wanting the best students. Many stories are told from the period. James Clerk Maxwell, as his later brilliant career might indicate, was an unruly student given to following his own bent. On the first day of his final examination, Maxwell's coach observed to a colleague that he had never sent a student "in" so ill-prepared: "Yet," added the coach, "I have some confidence in him, for he is incapable of thinking wrongly physically."

Over the half century from 1860, the coaching system developed into the college lecturing system, and college lecturing then developed into university lectures. The best coaches probably had little need of college patronage. They became well off financially from their own efforts, likely enough nurturing some envy in less fortunate colleagues, who compensated by obtaining a supply of students through individual colleges, of which they became fellows. I suspect there was a clash of interest, because a measure of contradiction developed that persisted into my own day. On the one hand, there was the one-to-one relationship of coach to individual student, in which the student took along his difficulties to be ironed out by the coach, and, on the other hand, there was the situation of a lecturer talking at students without much reference to their individual difficulties. It was the difference between a hand-made article and a mass-produced one.

There was still a one-to-one mystique during my undergraduate days in the 1930s, but, in practice, it turned out to be one-to-eight. When I became a college lecturer myself in the late 1940s, the normal situation was one-to-six for science students and one-to-three for mathematics students. Either way, I never felt the arrangement to be satisfactory. Students in a group are never sufficiently uniform to be treated as if they were an individual. If questions are asked, the tendency is for the best of a group, or the most extroverted, to do all the asking, with the rest probably getting less out of the proceedings than with the supervisor in the driving seat. Tutorials of the kind practiced in some universities are different and probably better. In a tutorial, the students are set questions to work by the supervisor, who circulates from one to another, helping when a student gets into difficulties. The problem with tutorials at Cambridge was

that our examination questions were just too hard for the method to work. Nevertheless, an attempt at it would have been salutary, for a pretense developed in which everyone was at fault, a pretense in which students appeared to be much better than they were—actual examination results were mostly a shock to supervisors. If we had seen the situation clearly, we would have realized that it was essential to begin with questions that were much less difficult.

The most satisfactory memories I have of undergraduate supervision are of exceptional circumstances that led to the old style of coaching. One week, five out of six science students in a group were absent with flu. The remaining chap was a scholar of the college, one of our best students. He asked about a relatively simple problem in elasticity, which I managed to solve. In the ordinary way of things, this would have ended it. The group of six-plus-supervisor would have moved on to something else. But now I was able to see that the lad had not understood. So I worked the problem again from a different point of view, and again I found the student was not understanding. At that stage, I revealed my limitations as a teacher, for I began to get exasperated. How could a chap of such ability not see something so simple? Our somewhat fast and furious conversation stopped suddenly. The student's face went red, and he began to laugh. It was a fine example of the penny dropping suddenly. Evidently, the lad had been suffering from the sort of logjam in the brain that sometimes happens with all of us. Once the jam was gone, the situation was instantly clear to him. Only in the old system could this essential kind of correction happen. In the way we had it from my own student days onwards, there was normally no way to get things right once they had gone wrong, a situation that every student comes to fear.

One post-Easter term, I was fortunate to have an ideal student on a one-to-one basis. He never asked me to look at a problem without having made a serious shot at it himself, and he never came without bringing his attempt clearly set out on paper. Usually, it was only a detail he wanted to discuss. In the few cases where he had become stuck, as soon as I suggested a move he had overlooked, he had no wish for me to continue; he wanted to go away and finish the job by himself. It was no surprise when he became the top student of the university in the final examinations.

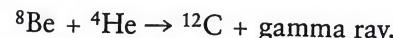
Coaching in the nineteenth century did not extend to research. For research, you were on your own, with neither college nor university to guide you. Harold Jeffreys, the St. John's mathematician, told the story of a student who did very well in the mathematical tripos (judging from the details, I think the student must have been Jeffreys himself). After the examination results were out, the student went to see one of the college lecturers in St. John's, the expert in elasticity, A. E. H. Love. After con-

gratulating the student, Love then kept his mouth shut, and the student also kept his mouth shut. When an embarrassingly long time had elapsed, Love gave way by congratulating the student again, when, once again, Love kept his mouth shut. Eventually, the student himself broke the second embarrassing interval by asking Love if he had any suggestion as to what might be a suitable research topic. Love thought for a moment and then said: "Young man, do you think that, if I were fortunate enough to have an idea, I would give it to you?"

The key word here is "give." In the old system, a student had no right to expect to be helped in research, but, once he heard of an idea, it was his to fasten on to. It could not be taken away or even shared by someone else. This explains why there were very few shared pieces of research of a mathematical nature in the nineteenth century. The system came through into my time, but with a crucial addendum that greatly loaded the scales in the student's favor. The degree of Ph.D. was introduced in the 1920s for the benefit of research students who came from outside the university, particularly those from the United States. It was an innovation that Maurice Pryce deplored as a debasing of the coinage. Maurice showed his contempt for the thing by satisfying all the requirements for the Ph.D. but then not bothering to join a congregation in the Senate House to have the degree officially awarded. It will be recalled how he persuaded me to do likewise—not for high-flown moral reasons, I fear, but because, in that way, I avoided having to pay income tax on a generous studentship that I had unexpectedly been awarded. Anyway, after introducing the Ph.D., the university required all registered research students to have an official supervisor, and, from then on, A. E. H. Love's option of keeping his mouth shut was taken away. The research student could now expect to receive ideas from a supervisor, and, once received, they became his or hers. I could not object myself when it came to my turn to provide ideas, because I had profited myself from the system when, in 1936, I had become a student of Rudolf Peierls. It was due to the good ideas that Peierls had given me that I had been able to get ahead so fast during the following two years. There were other occasions in which the situation was far from being fair, however. At the end of 1930, E. C. Stoner, later Professor of Physics at Leeds University, published a paper in *Philosophical Magazine* in which he showed that he had obtained the critical stellar mass known nowadays as the Chandrasekhar limiting mass. Stoner used a mean-value theorem for the astronomical part of his paper rather than a full stellar integration, which led to his numerical answer being slightly different from the eventually accepted value. But it was an exceedingly important result, and it can hardly be doubted that it was Stoner's supervisor, Ralph Fowler, who lay behind it,

the Fowler whose lectures on statistical mechanics I had taken over in 1945. If this is correct, it seems unfair that Fowler should be accorded no part in an important and widely acclaimed result.

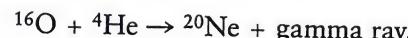
The satisfaction for a supervisor lay in the student making a good job of what he was given. I never had any complaint on this score—except once. The exceptional case happened in the period 1949–1951. I gave what eventually turned out to be a good idea to a student who decided to ditch the problem about two-thirds of the way through. But, since the student did not cancel his Ph.D. registration, I had no option but to wait until the registration lapsed, or until the student completed the Ph.D. requirements in some other topic. The problem in question had already been foreshadowed in Hans Bethe's 1939 paper on the conversion of hydrogen to helium. It was to make the step from helium to carbon at higher temperatures than in main-sequence stars, but not so hot as in the statistical calculations I had done in 1945–1946. The first maneuver was to calculate the statistical abundance of beryllium-8 in the reversible reactions ${}^4\text{He} + {}^4\text{He} \rightleftharpoons {}^8\text{Be}$. Then one had



but also



and so on,



It was this chain that I set the student to calculate and with which he became fed up. Then, in 1952, Ed Salpeter of Cornell University published a paper on the synthesis of carbon from helium, and I became thoroughly frustrated by the system, my frustration serving as a critical link of the chain that led to the discovery of the 7.65 MeV level in carbon-12.

The second link came with the meeting of the International Union of Astronomy in Rome in the summer of 1952. Increasing prosperity in Italy could be measured by the nature of the traffic in Rome. In 1952, it was by motor scooter, when it was not by train or bus. By 1958, it was by the little Fiat 500 c.c., and so on up and up the scale. In 1952, then, the piazzas were a bedlam of scooters. Those were the days in which Italian women were not supposed to appear in public with bare arms. Bernard Miles told me a story belonging also to those times, but not in Rome, in North Africa. He was doing a film with a Scandinavian blonde of classic features and statuesque proportions. They walked one day in a town, she with bare legs and arms. Arab males began to mutter, and a crowd around them began to advance in what looked like a tight situation. The Scan-

dinavian blonde waited until the nearest male was within two yards. Then she closed the gap, swung an arm, and lifted the fellow clean off his feet. Bernard said he expected knives to be out at a trice. But this didn't happen. The crowd just suddenly became quite silent, the men feeling the way male spiders must feel, I suppose.

Anyway, Walter Baade was there in Rome at the 1952 meeting of the International Astronomical Union. Walter was then the Chairman of the Commission on Extragalactic Nebulae (galaxies). It was typical of his disregard for bureaucratic detail that he had overlooked the need for a secretary to take the Commission's minutes. So what does Walter do but ask me, on the spot, to become secretary, without bothering to verify that I was a member of the Commission, which I wasn't.

My minutes were eventually to have importance in establishing Walter's priority for breaking a fifteen-year-old logjam concerning the distance scale of the galaxies, which affects the so-called age of the Universe. Edwin Hubble's estimate for the age had been 2 billion years, and Baade's was 3.6 billion. Eventually, the age was to rise higher and higher, attaining a maximum of about 15 billion. But nothing in the later increases caused quite the sensation of Baade's first jump from 2 to 3.6. My own increase to above 10 billion passed by quickly enough in 1958–1959, although the increase to the range 12–15 billion by Willy Fowler and me in 1960 attracted a little more notice.

Baade's announcement was made orally. When one makes a clear-cut statement to a considerable body of people, it seems impossible that anybody could be in doubt about the precise circumstances of the announcement. Yet experience shows that very few members of an audience ever do remember the exact details of such situations. Within a year, it becomes impossible to recover what was said, unless explicit details have been written down. This failure of the human memory opens the floodgates of plagiarism, permitting a subculture to flourish in which certain individuals make the round of international meetings picking up bits and pieces that they then proceed to represent as their own. It is a maxim of biology that, wherever a niche of survival can exist, it does. It was a maxim that the unworldly Baade had not learned. Surprising as it may seem, attempts were made to rob him of his priority over the age of the Universe, and, at the pinch, it was my minute that saved the situation.

This was another link in the chain leading to the 7.65 MeV level in carbon-12, for Baade sat on the combined astronomical steering committee of the Mount Wilson Observatory and the California Institute of Technology. It was to his good offices there, and to our meeting again in Rome, that I ascribe the invitation I received, in the autumn of 1952, to spend the first three months of 1953 at Caltech, to be followed by two

months at Princeton University. Because living conditions in the United States were then so much superior to those in Europe, such invitations were greatly prized.

When the autumn term of 1952 ended, I flew to New York, where I made my first contact with George Jones at Harper Brothers, then on East Thirty-third Street, George being the American publisher of *Nature of the Universe*. I went then to Princeton, where I made arrangements for the lectures I was to give there a few months later. In Princeton, under Martin Schwarzschild's tutelage, I bought a car. My intention was to drive to California more or less by the route Frank Westwater and I had flown over eight years before: First, the Pennsylvania Turnpike to Washington, D.C.; then the Blue Ridge as far as Asheville; through Tennessee and over the Mississippi River at Memphis; then the long drive across the prairies to Meteor Crater (near Winslow, Arizona), the Painted Desert, and the Grand Canyon. I spent Christmas Day at Phantom Ranch at the bottom of the Grand Canyon, winning a few dollars from a party of mule drivers in a card game I found difficult to understand. Then I drove across the Mojave Desert and over the Cajon Pass to San Bernardino, and thence mostly through orange groves to Pasadena, halfway between Christmas and the New Year.

In preparation for the lectures I was to give at Caltech, I began once again to work through the details of the production of carbon from helium. But the details wouldn't go properly, not unless the carbon atom had a state at an energy level where, according to experiment, there did not seem to be a state. The trouble was that, otherwise, carbon was scoured out as it was slowly produced, scoured out to oxygen by this reaction:



Eventually, I felt so convinced about the point that I went to see Willy Fowler. I explained the situation and asked if there was any possibility of an experimental oversight. Fowler has said, in later years, that his first impression was that I had somehow gone a long way off my mental compass bearings, but I can't remember any such suggestion being made overtly. Instead, I remember him calling a small group of experimentalists into his office, the argument being repeated for their benefit, and there being a long technical discussion of whether the experimental methods used thus far might have missed the state I was looking for. The outcome was indeed that an even state of zero spin could possibly have been missed. With this information, together with my calculation of the actual energy of the state, about 7.65 MeV above the lowest energy state (known as the ground level), the group was able to design a new experiment to

look for it. As I recall, it took about ten days to verify the prediction. There was indeed a state in carbon-12 and at about the energy level I had predicted. The day I heard the result, the scent of the orange trees smelled even sweeter.

I suppose the nearest one can come, in the ordinary way of things, to reproducing the way a scientist feels, when a prediction of his is being tested, is to be in court, in dock, with the jury out. Except, of course, the prisoner in dock knows already whether he is really guilty or not. In court, the prisoner hopes the jury gets it right if he knows he's innocent, and he hopes the jury gets it wrong if he knows he is guilty. In physics, on the other hand, the jury of experimentalists can be taken always to be right. The problem is that you don't know whether you're innocent or guilty, which is what you stand there waiting to learn, as the foreman of the jury gets up to speak.

After some struggle in classically minded quarters, the world came to believe in the general theory of relativity on the basis of only a single predictive test, the deflection of the direction of starlight passing close to the surface of the Sun. Einstein is reported to have said, after the test had been made successfully, that he never felt much strain while it was going on. Perhaps the five years or more needed to make the test helped to lighten the tension, but, even so, if indeed the report is true, Einstein must have had nerves of steel. Even in my small case, I felt the hot wind on my neck as I crept each day into the laboratory, escaping into the open air with relief as, for a period of two weeks, each day passed by without a result.

When the modest euphoria of success had faded, I was left in some awe at the broad picture that had emerged. It involved beryllium-8 and oxygen-16 as well as carbon-12. Chance coincidences appeared to be involved in all three of these nuclei, if the twenty or so indispensable elements (that is, the elements apparently required for the existence of life) were to be synthesized successfully in nature. Calculations of stellar structure led to the conclusion that, were the beryllium-8 nucleus stable, the production of carbon from helium would always proceed so explosively that stars in which "helium-burning" occurs would be blown violently apart, thereby making the synthesis of such elements as magnesium, sulfur, calcium, and iron impossible. Because the beryllium-8 nucleus is actually unstable, it splits quickly apart into two helium nuclei, making helium-burning a safe, slow process. What happens is that a beryllium-8 nucleus forms and then splits apart, with statistical balancing set up in the reversible reaction ${}^8\text{Be} \rightleftharpoons {}^4\text{He} + {}^4\text{He}$, the concentration of beryllium-8 in the statistical balance being very low, making carbon production proceed only slowly by the reaction ${}^8\text{Be} + {}^4\text{He} \rightarrow {}^{12}\text{C}$. Too slowly, I found, unless some special property was added in order to favor the latter re-

action. The special property was the existence of the state in carbon-12, which was required to be present at such a level that the reaction would be in resonance at an energy nearly equal to the sum of the energies of beryllium-8 and helium-4. This was how the explicit value of 7.65 MeV for the level was calculated. But this was not all. It would be no use having carbon produced at a satisfactory rate if all of it were lost in the formation of oxygen in the reaction $^{12}\text{C} + ^4\text{He} \rightarrow ^{16}\text{O}$. On the other hand, it would be no use either if *no* oxygen were formed—at any rate, at least as far as life is concerned. Not only had carbon production by the reaction $^8\text{Be} + ^4\text{He} \rightarrow ^{12}\text{C}$ to proceed in a suitably controlled way, but so had the reaction $^{12}\text{C} + ^4\text{He} \rightarrow ^{16}\text{O}$. This, I was able to show, was dependent on the oxygen nucleus *not* having a level in which oxygen production could proceed in a resonant way, the position for oxygen being the opposite of that for carbon. The experimental evidence for the nuclear structure of oxygen showed there was actually a state, very close to the danger point, given by the sum of the energies of carbon-12 and helium-4, at an energy excitation of 7.19 MeV, just a little above the experimental state at 7.12 MeV. Technically, this meant that the situation was safe—but just barely so. With 7.12 MeV below 7.19 MeV, there could be no resonance. But, if the energy of the state had been above 7.19 MeV, all would have been lost. Essentially, all carbon would have gone to oxygen in short order. So the situation turned crucially on two numbers very close to each other being just the right way around. Were they the other way, there would be no life.

All of this suggested to me what I suppose might be called profound questions. Was the existence of life a result of a set of freakish coincidences in nuclear physics? Could it be that the laws of physics are not the strictly invariant mathematical forms we take them to be? Could there be variations in the forms, with the Universe being a far more complex structure than we take it to be in all our cosmological theories? If so, life would perforce exist only where the nuclear adjustments happened to be favorable, removing the need for arbitrary coincidences, just as one finds in the modern formulation of the weak anthropic principle. Or is the Universe teleological, with the laws deliberately designed to permit the existence of life, the common religious position? A further possibility, suggested by the modern strong anthropic principle, did not occur to me in 1953—namely, that it is our existence that forces the nuclear details to be the way they are, which is essentially the common religious position taken backwards. Before ridiculing this last possibility, as quite a few scientists tend to do, it is necessary, as I pointed out before, to explain the condensation of the universal wave function through the intervention of human

consciousness. While this could be seen as a matter for philosophical discussion, I suspect its resolution will eventually come from exact science.

I set to work immediately to broaden the base so favorably revealed. Before I left Caltech in March of 1953, I had found the additional processes of synthesis that are referred to nowadays as carbon-burning and oxygen-burning, which, in the 1957 paper of Burbidge, Burbidge, Fowler, and Hoyle, were referred to as the alpha-process. Before I left Caltech, I had also made a preliminary draft of a paper that was to appear the following year in *Astrophysical Journal, Supplement Series*, under the title “I. The Synthesis of the Elements from Carbon to Nickel.” The “I” in the title meant that all neutron processes were left over for a second paper, which was to be part II. In the event, part II never appeared. With many improvements from others, it passed through into the 1957 paper of Burbidge, Burbidge, Fowler, and Hoyle.